

A REVIEW OF BAUM'S REVIEW OF CONCEPTUAL FOUNDATIONS OF RADICAL BEHAVIORISM

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Baum expressed numerous concerns about my *Conceptual Foundations of Radical Behaviorism* in his review. If his review were an independent submission and I were an independent referee, I would recommend that his review be rejected and that he be encouraged to revise and resubmit, once he has studied the field a bit more and clarified for himself and journal readers several important matters. I outline two sets of concerns that he might usefully clarify in his revision: (a) the important contributions of B. F. Skinner to a book about radical behaviorism, and (b) the nature of private behavioral events. In particular, the methodological behaviorism inherent in Baum's position needs to be resolved.

Key words: radical behaviorism, methodological behaviorism, behavior analysis, folk psychology, dispositions, B. F. Skinner

In his review, Baum (2010) argued that my *Conceptual Foundations of Radical Behaviorism* (Moore, 2008) is incomplete, distorted, and woefully out of date, among milder descriptors. I am reminded of Skinner's comments about Chomsky:

I have never been able to understand why Chomsky becomes almost pathologically angry when writing about me but I do not see why I should submit myself to such verbal treatment. If I thought I could learn something which might lead to useful revisions of my position I would of course be willing to take my punishment, but Chomsky simply does not understand what I am talking about and I see no reason to listen to him. (as reported in Andresen, 1991, p. 57)

The editors generously granted me an opportunity to respond to Baum's review. I do so by reviewing his review. If his review were an independent submission and I were an independent referee, I would recommend that his review be rejected and that he be encouraged to revise and resubmit, once he has studied the field a bit more and clarified for himself and readers two important matters. The first is his claim regarding the "mistakes"

of B. F. Skinner. The second is his treatment of private behavioral events.

Baum's review contains many criticisms. If I do not comment below on some specific criticism, I ask readers not to assume the criticism is so essentially valid as to be unanswerable. Rather, I ask readers to recognize that I only wish to respect their patience with a reply of manageable proportions. An examination of the two matters I identify above is sufficient to make the case against Baum's review.

THE MATTER OF SKINNER'S "MISTAKES"—AND MINE

One of Baum's principal criticisms is that my book did not pay sufficient attention to Skinner's "mistakes." Baum also suggested that I apparently manufactured some of my own. My mistakes presumably came about when I uncritically attempted to add to Skinner's position, thereby elevating Skinner's work to the status of scripture.

Despite Baum's concerns, the book was not written as Skinner hagiography. Rather, I simply recognized that Skinner's writings can be used as a vehicle to convey important points in a science of behavior. Let me now examine some of Baum's specific concerns as they relate to Skinner's contributions.

Punishment

One case Baum cited relates to punishment, though here Baum apparently wanted me to spend more time instead of less on what

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Skinner said. As many readers know, William Estes, who studied with Skinner at Minnesota, showed that when the punishment operation is in effect, rate of responding may well decrease. However, after punishment is discontinued, responding recovers rather rapidly to prepunished rates (Estes, 1944). The rapid recovery of responding influenced Skinner's thinking about punishment a great deal.

Skinner (e.g., 1953, pp. 182–193) embraced a negative reinforcement interpretation to explain the changes in behavior associated with punishment. He suggested that when punishment was actually in effect, an organism simply avoided making the punished response. After punishment was discontinued, there was no longer any reason to avoid the target response. Consequently, the organism simply resumed responding at its original rate. Hence, Skinner concluded that punishment should be considered as ineffective in directly reducing or eliminating behavior over the long term, for example, via a reduction in the underlying “strength” of the response. Perhaps it was more useful to regard any reduction in responding as indirect and transient.

Baum correctly noted that my book did not explicitly point out that Skinner had argued punishment was ineffective in reducing or eliminating behavior over the long term. Baum also correctly noted that others produced a large body of orderly data that challenged a negative reinforcement interpretation (e.g., Azrin & Holz, 1966). These data suggested that punishment can indeed be effective, even in the long term, and can indeed reduce responding directly. However, several other parameters, such as the way the punishment is implemented and maintained, the intensity of the punishment, and the nature of the reinforcing conditions that engender the responding in the first place, need to be taken into account. An interpretation based on the direct, response-reducing effectiveness of punishment is presented in my chapter 6, along with mention of several of these other parameters.

If the purpose of my book was to critically analyze the entire corpus of Skinner's written work, then my book could well have gotten into how Skinner's view differed from the interpretation above. However, this sort of personal, critical analysis was clearly not the purpose of the book. I am curious why Baum

apparently assumed it was or even should have been, and why Baum therefore asserted my book must be considered deficient because it did not explicitly document Skinner's personal view of punishment.

Equivalence

Chapter 9 of my book discussed equivalence and other forms of derived relational responding, which Skinner (1957, chapter 14) anticipated but did not explore in any detail. Interestingly, Baum stated that about the only “modern development” I included in my book was derived relational responding, which I singled out for praise. His implication here seems to be that including this development was one of many palpable examples of my bad judgment and my personally limited intellectual resources because other behavior analysts consider derived relational responding a retreat into mentalism. Perhaps Baum is interested in the reservations about Relational Frame Theory in Moore (2009), which he could have consulted prior to his review if he believed it was at all useful to accurately report my position for readers.

Reinforcement

I confess even less certainty about the basis for Baum's implication that his molar view of reinforcement should be regarded as uncontestedly superior to Skinner's, by virtue of its molar orientation, so Skinner's view should be regarded as some sort of mistake. Clearly, Skinner did stress the importance of temporal contiguity: “So far as the organism is concerned, the only important property of the contingency is temporal. The reinforcer simply *follows* the response. How this is brought about does not matter” (Skinner, 1953, p. 85). Important to emphasize is that reinforcement selects a class of behavior, and that it can select more than just an atomistic bit of behavior according to a “stop-action” principle, as Baum implied. Questions pertaining to the nature of the response unit that is selected and its class boundaries are ultimately empirical questions. Patterns of behavior extended over time, such as implied in everyday words like purpose and intention, are clearly within the selectionist view of radical behaviorism, notwithstanding Baum's challenges (e.g., Skinner, 1953, pp. 85 ff.: section titled “Goals, purpos-

es, and other final causes"; Skinner, 1974, pp. 56–59: section titled "Purpose and intention"). The extent to which contrived relations in a given contingency can supersede the "natural lines of fracture" to produce individual response units that are "quantitatively mutually replaceable" is similarly an empirical question (Skinner, 1935). Overall, my book's treatment of consequences such as reinforcement and punishment relied to a great extent on Catania (2007).

Why Mistakes?

In any event, I am uncertain why Baum wanted my book to document these or other matters as Skinner's "mistakes," if indeed they actually were all mistakes. Perhaps Baum wanted me to somehow delimit credit to Skinner and more formally recognize what those who differed from Skinner have said, although for what reason I am not clear.

Moreover, on Baum's view my mistakes came about because I largely equated behavior analysis and radical behaviorism with what Skinner said through the middle of the 20th century and paid insufficient attention to what others have said since then. Baum suggested I should have cited other areas of research or individuals as an antidote to Skinner.

Choice of References

That Baum (2010) would argue I should have cited Staddon's research to remedy my uncritical reliance on Skinner is an interesting point. Readers may recall that Baum (2004) previously criticized Staddon (2001) in much the same language as he did my book, although Baum then apparently agreed with Staddon that the continuing influence of Skinner was unfortunate, and sympathized with most of Staddon's criticisms of the contemporary scene and his essential points. Nevertheless, for Baum, the pictures of behavior analysis that emerged from the two books, though different, were both defective because they did not emphasize the right individuals and sources. Staddon emphasized himself too much. I emphasized Skinner too much, but then for Baum's tastes curiously did not emphasize Staddon enough, even though when Staddon emphasized his own research, Baum said he shouldn't have done so.

It seems that for Baum, both Staddon and I should have cited more of Baum and Rachlin

to give an accurate picture of contemporary behavior analysis, and in my case, ostensibly remedy Skinner's ostensible mistakes. I cannot speak for Staddon's choice of references. For my part, I deliberately did not see the need to further cite Staddon or Baum or Rachlin, as their books are already available. So far as I know, all three are perfectly capable of promoting their own views, and they have not been reluctant to do so. Besides, I was not conducting a "box-score" analysis of the field. Of course, I cannot say whether Baum felt personally peeved because I did not cite him and his differences with Skinner. I do look forward to the day when Baum will cite my work, at least in something other than pejorative terms.

To be fair, I need to emphasize that Baum also suggested I could have done more with such sources as Chiesa (1994), Lattal and Chase (2003), Lee (1988), O'Donohue and Kitchener (1999), and Todd and Morris (1995). Again, these sources have previously been reviewed in behavioral journals and are readily available. In fact, I reviewed some of them myself, and contributed chapters in others (Day & Moore, 1995; Moore, 1989, 1995a, 1995b, 1999, 2003). Actually, I did cite Place's (1999) chapter from O'Donohue and Kitchener in my chapter 18. I am sorry that Baum apparently missed this citation.

Nevertheless, if Baum criticized my book because it narrowly emphasized a few of Skinner's concepts and omits Skinner's "mistakes," I am unclear why his criticism seemed to emphasize only a selected set of sources or issues as alternatives, namely, those derived from his personal world view predicated on a quantitative molar orientation. Even if my book did take up the topics he preferred, is there nothing with a more local-immediate analytic orientation (as opposed to molar-correlational) that would have a place in a book that suits Baum's tastes? Would a book that suits Baum's tastes also need to include something about applied behavior analysis? Moreover, I am unclear how the book would be shorter by a third if it included all the material Baum recommended from his contemporaries in the Harvard Department during the 1960s, let alone the others he tags.

In the final analysis, Baum has regrettably failed to make an independent case for his criticisms. For example, there are more than

250 different references in the book. Of these, more than 70% carry dates after 1960, and nearly 40% carry dates after 1980. So much for his insistence that the book is relevant only to the middle of the 20th century. Whatever Baum was responding to in his review, it apparently wasn't limited to the actual content of the book. His criticisms certainly need to be clarified in his revision.

THE MATTER OF PRIVATE BEHAVIORAL EVENTS

Baum (2010) further argued that my book overlooked perhaps Skinner's biggest mistake: "conceding that accounts of public behavior are incomplete without private events and that private events may cause public behavior" (p. 121). I am unsure whether Baum counted completeness and causation here as two different issues or just different aspects of the same issue, but I need not dwell on how he counted versus how I count. Nevertheless, I wonder whether Baum actually understands the radical behaviorist view of private behavioral events. I would now like to examine the radical behaviorist view, and Baum's negative reaction to it, in some detail.

Radical Behaviorist View of Private Behavioral Events

The topic of private behavioral events concerns how private stimulation exerts discriminative control over our subsequent behavior, whether that behavior is verbal or nonverbal, public or private (see Moore, 2001). Absent such a behavioral account, traditional mentalism or even dualism prevails. Thus, a behavioral account of private behavioral events is of paramount importance in a science of behavior.

Radical behaviorists conceive of two types of private behavioral events. In one type, radical behaviorists address the influence of private stimulation from internal conditions or states of the body. Here we are interested in the processes by which this private stimulation occasions our verbal behavior. This is the traditional type of "verbal reports" concerned with "the use of subjective terms." In another type, radical behaviorists address private stimulation from our own private verbal or nonverbal behavior. Here we are interested in the processes by which this stimulation occasions the

behavior that follows. This is the traditional matter of "thinking." With regard to vocabulary, in what follows I will occasionally use the term overt as synonymous with public and the term covert as synonymous with private.

Let me now more specifically consider the first type of private behavioral event. In typical circumstances, verbal behavior develops when the verbal community differentially reinforces a response contingent on the presence of a discriminative stimulus. The differential reinforcement can range from the approval inherent in ordinary discourse to actually receiving some tangible consequence, such as receiving the salt at the dinner table after requesting it be passed. Important in this relation is that both speaker and verbal community are in contact with the same discriminative stimulus, so that (a) the verbal community can maintain the appropriate consistency in its reinforcing practices and (b) the discriminative stimulus can then become the appropriate occasion for the speaker's emitting the verbal behavior in question in the future.

However, the verbal community operates with a handicap when it comes to verbal behavior related to private stimulation from internal conditions or states of the body: How does the verbal community differentially reinforce talk related to this stimulation, so that speakers may later talk about it in a reasonably consistent fashion, when only the speaker is in contact with this stimulation and not the verbal community? In everyday language we can say that the verbal community doesn't know when the appropriate private stimulation is present or absent. We might call this problem the "problem of privacy" (Skinner, 1945). The verbal community obviously does solve the problem, given that we obviously do learn to talk about our aches and pains, or joys and sorrows, in ways that affect listeners.

The answer is that the verbal community can differentially reinforce responses based on public states of affairs. These public states of affairs are accessible to both speaker and verbal community, and are correlated with the private stimulation. Control then develops in an original situation based on public stimulation, and then transfers to the accompanying private stimulation, so that eventually the preponderance of the control resides with the private form. Of course, these processes vary a great deal across speakers. The result is

that verbal reports of speakers may similarly vary a great deal.

To use pain as an example, the verbal community may initially reinforce pain talk when speakers put their hands to the area that is the source of the pain—a public collateral response that is correlated with the pain. In addition, the verbal community may initially reinforce pain talk when some object has visibly struck a speaker, resulting in inflammation or tissue damage—a public accompaniment that is correlated with the pain. Finally, if control by private stimulation related to bodily states or conditions has already developed, then that control may generalize from existing to new forms of private stimulation. Given that we have already learned to talk about the pain we experience when we step on a nail as sharp, we talk in a similar way about the pain we experience when we stub our toe as sharp. The result is that speakers end up being able to talk under the control of internal conditions and states that are accessible to only themselves.

Now let me more specifically consider the second type of private behavioral event. Our central question here is: How does private stimulation from our own covert verbal or nonverbal behavior acquire discriminative control over the behavior that follows? The answer begins with the recognition that behavior is usually acquired at the overt level. However, through the action of environmental variables and relations, the behavior may then recede to the covert level. Covert behavior is executed with the same organs as overt behavior, but on a reduced scale, perhaps even at incipient or inchoate levels.

We may then ask: Why does the behavior become covert? The answer here is that the overt form may be punished, necessary environmental support may be absent, or an actor may simply be able to respond faster covertly rather than overtly. If the overt form of the behavior was a link in a chain of responses that contributed to discriminative control over subsequent behavior, then presumably the covert form will function similarly.

We make contact with our covert behavior through our interoceptive and proprioceptive systems. This private stimulation is also present when an individual behaves overtly. Consequently, it will gain some measure of discriminative control in those circumstances. Many usages of the term 'thinking' reflect situations

wherein the stimulation from one instance of behavior—and the behavior need not even be covert—affects subsequent behavior (Skinner, 1953, chapter 16; 1957, chapter 19). Once discriminative control is acquired, the control can generalize to new circumstances. As before, these processes vary a great deal across individuals. The extent to which individuals come under the control of this sort of discriminative stimulation may similarly vary a great deal.

For radical behaviorism, when a private behavioral event does contribute functionally to public behavior, some prior experiences are necessary for the private event to do so. Nevertheless, responding with respect to private or covert stimuli is lawful and alike in kind to responding with respect to public or overt stimuli. Private stimuli may be interpreted as simply additional independent variables in the same dimensional system as public stimuli.

The forgoing differs greatly from Baum's story about private events. Baum's comments at the beginning of this section seemed to suggest that for Skinner, accounts of behavior are incomplete without private events. Regrettably, the way Baum portrayed Skinner's position is ambiguous. To suggest that Skinner said accounts of all behavioral events are *necessarily* incomplete without an appeal to private events would be dead wrong. It is an empirical question whether private events accompany public events and therefore are appropriately included in accounts of public events. To suggest that private events can complete accounts of some behavioral events but are not necessary for accounts of other behavioral events would be correct. Indeed, to so suggest supports Skinner's argument.

Baum also seemed to suggest that for Skinner, private events cause public behavior. Again, the way Baum portrayed Skinner's position was ambiguous. To suggest that Skinner said private events were *necessarily* initiating or mediating causes of public behavior would be dead wrong. To suggest that private stimulation may contribute to causation of public behavior in the sense of discriminative control, where this contribution is in turn conditional on further relations, would be correct. As before, to so suggest exactly supports Skinner's argument as it has appeared elsewhere (e.g., Skinner in Catania & Harnad, 1988, pp. 215, 354, 482, and 486).

Baum's statements about private events in his review need to be comprehensively clarified if he chooses to revise and resubmit his comments. But there are other aspects of Baum's approach to private events that are equally troublesome and therefore demand attention.

Functional Analysis of Mental Talk

If mentalism is largely verbal behavior, then even mentalistic verbal behavior can be explained and understood in terms of the conditions that promote it. In short, something occasions mental talk. When people engage in mental talk, a functional analysis of their verbal behavior (although more likely interpretation than formal experimental analysis) is probably going to identify that any of several factors, either singly or in combination, could occasion that talk (e.g., Moore, 2001). The mental talk could actually be about private behavioral events, as outlined above. Mental talk could be about physiology, as when speakers talk about the functional organization of underlying neurophysiological systems. Mental talk could also be about dispositions. Dispositions are robust conditional probabilities of engaging in some particular form of behavior, given some particular form of antecedent stimulation. A common example of dispositional usage is the "intentional idiom," when we talk in everyday language using such terms as intentions, beliefs, and desires. For radical behaviorism such terms are simply descriptive terms occasioned by the extent to which certain antecedent variables and relations, perhaps including discriminative control from an individual's own verbal behavior, have resulted in the high probability of a certain form of behavior. Ryle (1949) is often cited for his dispositional analyses, although he also accepted episodic uses for so-called mental terms. In principle, the several instances of mental talk outlined in this paragraph are not especially troublesome.

The trouble comes with other sources of control over verbal behavior. Some mental talk exhibits simple intraverbal control, or generalizations based on grammatical and syntactical patterns of the English language, as when adjectives or adverbs are turned into nouns in processes sometimes called reification or hypostatization. Other mental talk could exemplify the traditions of folk psychology. In

folk psychology we take descriptive terms from ordinary language and elevate them to causal mental entities—we remember the name of a person we meet on the street because of our mental process of "remembering," we recognize what move to make on a chess board because of our mental process of "perceiving," and so on. Folk psychology also comes into play when we interpret terms of the intentional idiom—beliefs, desires, intentions, and the like, as reflecting actual mental entities instead of dispositions. The mental elements of folk psychology are largely explanatory fictions of cultural origin, maintained by the social reinforcement of talking and explaining behavior in conventionally approved ways. These sources of control are troublesome because under their influence, we cannot effectively explain natural behavioral events, whether the behavior in question is that of a pigeon pecking a key, a rat pressing a lever, or a child with a developmental disability acquiring a functional verbal repertoire.

Questions of Ontology

Skinner wrote a great deal about private events and mentalism. I include below a collection of representative statements from Skinner's writing that influenced me as I composed the verbal material on private events and mentalism in my book.

What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about is always the 'real' or 'physical' world (or at least the 'one' world) and that 'experience' is a derived construct to be understood only through an analysis of verbal (not, of course, merely vocal) processes. (Skinner, 1945, p. 293)

I am a radical behaviorist simply in the sense that I find no place in the formulation for anything which is mental. (Skinner, 1964, p. 106)

The basic issue is not the nature of the stuff of which the world is made or whether it is made of one stuff or two but rather the dimensions of the things studied by psychology and the methods relevant to them. (Skinner, 1969, p. 221)

The objection is not that these things are mental but that they offer no real explanation and stand in the way of a more effective analysis. (Skinner, 1969, p. 222)

It is a little too simple to paraphrase the behavioristic alternative by saying that there is indeed only one world and that is the world of

matter, for the word "matter" is then no longer useful. Whatever the stuff may be of which the world is made, it contains organisms (of which we are examples) which respond to other parts of it and thus "know" it in a sense not far from contact. (Skinner, 1969, p. 248) Scientific formulations do not change the nature of the things formulated, and a science of behavior neither ignores nor destroys the phenomena associated with introspection or any other form of self-observation. It simply represents them in other ways. (Skinner, 1969, pp. 267–268)

The present argument is this: mental life and the world in which it is lived are inventions. They have been invented on the analogy of external behavior occurring under external contingencies. (Skinner, 1974, p. 107)

I am, of course, a radical rather than methodological behaviorist. I do not believe that there is a world of mentation or subjective experience that is being, or must be ignored. (Skinner, 1978, p. 124)

But I preferred the position of *radical behaviorism* in which the existence of subjective entities is denied. I proposed to regard subjective terms as "verbal constructs, as grammatical traps into which the human race in the development of language has fallen." I was not concerned with the nature of the stuff of which the mind was composed: "The behavioristic argument is not that of the naive materialist who asserts that 'thought is a property of matter in motion,' nor is it the assertion of the identity of thought or conscious states with material [brain] states. (Cf. Boring.)" I thought it was a mistake to speak of knowledge of a *thing* possessed by a *mind*. "Try the operational method of knowledge. What can the human mind do peculiarly well?" (Skinner, 1979, p. 117)

[T]he reasons for the popularity of cognitive psychology ... have nothing to do with scientific advances but rather with the release of the floodgates of mentalistic terms fed by the tributaries of philosophy, theology, history, letters, media, and worst of all, the English language. (Skinner in Catania & Harnad, 1988, p. 447).

This collection of statements may strike readers as challenging. Some statements stress the "one world" as "real" and "physical" and "the world of matter," the view of mental life as an "invention," and the denial of the existence of subjective entities. These statements would seem to advocate a fairly robust monism.

Other statements stress that the "basic issue is not the nature of the stuff of which the world

is made" and that the objection is not to these things being mental. Despite the first set of statements, is it possible to interpret these other statements as implying that Skinner admitted a mental dimension that differs from a behavioral dimension does exist in some sense and that mental phenomena do initiate behavior, although mental phenomena can have no place in our formulations because they are not scientifically accessible to anyone, even in principle? I think not. I suggest that what Skinner did was to bracket the ontological question and relegate it to second place, rather than get into interminable ontological debate about his emphasis on the "only one world." In first place was the pragmatic question of the effectiveness of talk that appeals to events in two dimensions, relative to talk that appeals to events in one dimension, and this was sufficient. As Moore (2010) recently reviewed, the effectiveness is ultimately measured in terms ranging from prediction and control to "the discovery of uniformities, the ordering of confusing data, the resolution of puzzlement" (Skinner, 1979, p. 282).

Baum (2010) charged that the way I dealt with ontology, mentalism, and privacy is unnecessary, distracting, and gratuitous. For example, Baum cited my statement that "The mental dimension is rejected because it does not exist, and therefore when one talks of mental phenomena, one is actually not talking about phenomena from another dimension at all" (Moore, 2008, p. 431). Here I emphasized the functional analysis of verbal behavior, as I summarized it above. In reaction to my statement, Baum (2010) argued that "The problem with mental causes of behavior is *not* that they don't exist. Their existence or nonexistence cannot be demonstrated one way or the other, any more than the existence of a real world independent of our experience can be demonstrated, as George Berkeley (1685–1753) famously pointed out" (p. xxx). At face value, Baum's concern here is consistent with Skinner's statements that the central issue is not mental stuff but the effectiveness of mental talk. Nevertheless, Baum argued the public-molar approach such as found in Rachlin (1994) constitutes the science of mental life because it alone can explain talk about mental entities and events. I am afraid I just do not understand where Baum was coming from here.

On the one hand, Baum could have been affirming that mental or cognitive phenomena literally do exist, that these phenomena are in a different dimension than the behavioral dimension, and that these phenomena literally do initiate or mediate behavior. Baum could have been affirming that paying attention to these phenomena is not scientifically respectable, even in principle, because they are not publicly observable and cannot be agreed upon. They are ineffable and cannot be directly talked about. Therefore, Baum could have been affirming that scientists must remain silent on these phenomena and express the meaningfulness of statements about the phenomena in terms of inferences from publicly observable variables, such as temporally extended, publicly observable behavior. Observable behavior serves as an acceptable manifestation of the unobservable mental phenomena. Readers may recall that Rachlin, whose position Baum frequently lauded as exemplary, once seemed to embrace this view in a piece curiously titled "Mental, yes; private, no" (Rachlin, 1988).

On the other hand, Baum could have been emphasizing the pragmatic nature of the debate. He could have been bracketing the question of ontology, without conceding the mentalism of another dimension, and then calling for a comprehensive discussion of the behavioral variables and relations, whether public or private, that occasion mental terms. Indeed, this is what Skinner (1945) meant by an operational analysis of psychological terms. Ironically, though, Baum argued that Skinner was mistaken about private events, and we have seen Baum wanted me to acknowledge Skinner's supposed mistakes, as well as mine. I am concerned that Baum did not approach privacy in this way, given that Skinner acknowledged the direct relevance of private events *qua* private and Baum quite conspicuously rejected any consideration of private events *qua* private.

Private Events and the Requirement for an Explanation of Behavior in Objective Terms

Behaviorism is often defined as the view in psychology that requires an explanation of behavior to use only "objective" terms that refer to only publicly observable variables and relations. Correctly or not, the works of Watson (1913, 1925) and Meyer (1922) are often cited as the basis for this requirement.

According to this requirement, one reason private events and mental concepts could not be directly used in explanations of behavior was that they were not objective. They were not publicly observable, so they could not generate agreement, which was necessary for them to be considered as scientific. Where does a radical behaviorism stand on the requirement for an explanation in objective terms, especially in light of its stance on private behavioral events? More importantly for present purposes, where does Baum seem to stand?

Much of my book is devoted to resolving the complex issues that this topic raises, so I will not review the entire matter here. Suffice it to note that the earlier discussion of private events suggested the behavior of individuals may well be a function of certain independent variables accessible to themselves alone. For radical behaviorism, it is a mistake to dismiss this possibility simply because others are not in contact with these variables. We can incorporate these variables in the same fashion as we incorporate publicly observable variables. Doing so allows us to achieve "an ordered and integrated conception of nature" (Skinner, 1953, p. 258) in ways that do not ultimately entail mentalism. To be sure, the resulting account is an interpretation, based on applying known principles in new cases to which direct experimental manipulation has not yet been achieved. Nevertheless, it is no more troublesome than an explanation of earthquakes and continental drift in terms of plate tectonics or evolution in terms of descent with modification and natural selection. As our research techniques improve, so also will the scope and precision of our accounts of the relevant private behavioral processes. Here are two passages in which Skinner talked about the importance of recognizing the contribution of private stimuli:

[W]e cannot, as in the case of public stimuli, account for the verbal response by pointing to a controlling stimulus. Our practice is to infer the private event, but this is opposed to the direction of inquiry in a science of behavior in which we are to predict response through, among other things, independent knowledge of the stimulus. It is often supposed that a solution is to be found in improved physiological techniques. Whenever it becomes possible to say what conditions within the organism control the response 'I am depressed,' for

example, and to produce these conditions at will, a degree of control and prediction characteristic of responses to external stimuli will be made possible. Meanwhile, we must be content with reasonable evidence for the belief that responses to public and private stimuli are equally lawful and alike in kind. But the problem of privacy cannot be wholly solved by instrumental invasion. No matter how clearly these internal events may be exposed in the laboratory, the fact remains that in the normal verbal episode they are quite private.... There is, of course, no question of whether responses to private stimuli are possible. They occur commonly enough and must be accounted for. (Skinner, 1945, pp. 272–273)

The line between public and private is not fixed. The boundary shifts with every discovery of a technique for making private events public.... The problem of privacy may, therefore, eventually be solved by technical advances. But we are still faced with events which occur at the private level and which are important to the organism without instrumental amplification. How the organism reacts to these events will remain an important question, even though the events may some day be made accessible to everyone. (Skinner, 1953, p. 282)

Like many other researchers and theorists, Baum appears to adopt a version of a fairly orthodox position that requires all explanations of behavior to use only objective terms that refer to only publicly observable variables and relations. Baum's position seems to blend dispositional approaches with methodological behaviorism, and warrants more detailed consideration. We turn first to Baum's embrace of dispositional approaches.

Baum's Dispositions

In some cases, Baum's orthodoxy resembles the dispositional analyses of philosophical behaviorism. At issue for radical behaviorism and Baum's argument is whether talk of private events and mental terms should *always and only* be regarded as dispositional, that is, as meaningful only through reference to the temporally extended observation of behavior in a wider context. As a first example we return to the consideration of pain.

For radical behaviorism, pain is ordinarily regarded as a stimulating condition or state of the body. The condition may result from infection, inflammation, or injury. To be sure, observable behavior aimed at reducing the

painful condition may well become highly probable, via the process called negative reinforcement. Nevertheless, the behavior aimed at reducing the condition—a dependent variable, is not the same as the motivating condition itself—an independent variable.

Radical behaviorism agrees that persons in pain often do engage in some publicly observable collateral responses, such as moaning and groaning. There may also be overt public accompaniments to the pain, as when objects strike the body and produce tissue damage or inflammation. Sometimes public pain behavior or accompaniments are said to be a criterion for the conventional use of the term pain. Indeed, observers are inclined to infer actors are in pain when collateral responses or public accompaniments are present but not when they are absent. Nevertheless, the inference is from the point of view of an observer. As have others, radical behaviorism argues that actors in pain do not necessarily observe their own public behavior and then infer that they are actually in pain (cf. Baum's comments about "in corrigibility"). Moreover, as described in chapter 18 of Moore (2008), persons may even be in pain but not act like it (e.g., Putnam, 1980).

A second example we can consider is terms of the intentional idiom, such as beliefs, desires, and intentions. As we noted earlier, for radical behaviorism such terms are often variations of being dispositional. To adapt one of Skinner's examples, to say that individuals believe there is cheese in the refrigerator or that they desire or intend to go to the refrigerator to get a piece of cheese is typically to say that they have stated that a piece of cheese is in the refrigerator, the statement was a tact at the time it was emitted, and conditions are in effect that will result in their going to the refrigerator to get the cheese, with the tact contributing to the stimulus control over the response.

In short, radical behaviorism argues that when individuals speak of having a strong pain, they are ordinarily tacting a condition of the body with which they are in contact. They are not ordinarily tacting the high probability of a certain form of behavior as a function of certain antecedent conditions. In contrast, when individuals speak of having a strong belief, desire, or intention, they are ordinarily tacting the high probability of a certain form

of behavior as a function of certain antecedent conditions. They are not ordinarily tacking a condition of the body with which they are in contact.

It therefore seems to me that other private or mental terms would benefit from a case-by-case analysis, rather than Baum's blanket assertion that the only way that mental terms are meaningful is through reduction to public behavior, observed over a temporally extended period. As reviewed above, some terms may indeed reflect a dispositional usage. Others may in some sense reflect a state of the body, as in pain. Still others may reflect covert but nevertheless episodic, occurrent behavioral events in real time, as in many uses of the term 'thinking'.

Radical behaviorism may recognize the relevance of talking about internal states, but maintains those states are behavioral. It does not assign initiating or mediating causal status to them in any case. Baum's position is troublesome because he rendered all private or mental concepts as dispositional and ruled out that such things as pain statements might be functionally related to internal behavioral states or conditions. Baum also did not accept that purportedly mental terms may well relate to occurrent covert behavior. In light of his rejection of statements occasioned by internal states or the influence of covert behavior, he then did not address how control by private or covert stimulation develops. But there is more.

Baum's Methodological Behaviorism

Having reviewed Baum's embrace of what amounts to dispositional approaches, we now turn to his embrace of methodological behaviorism. Methodological behaviorism is the thesis that science can only consider public data because science requires agreement. Therefore, statements about private events, even if the events are not regarded as dualistic, cannot be a direct part of the data base of the science. All meaning is derived from public observations. Methodological behaviorism is sometimes taken as the outgrowth of the positions of Watson (1913, 1925) and Meyer (1922), mentioned earlier, although again the nature of Watson's contribution is extraordinarily complex.

Those familiar with the history of experimental psychology will recognize a close connection between methodological behavior-

ism and the principle of operationism. According to the principle of operationism, the meaning of an explanatory concept in science is synonymous with the corresponding set of operations by which it might be measured in the experimental laboratory. At first blush, this principle would seem to safeguard scientific explanatory concepts from extraneous and nonscientific considerations. However, E. G. Boring and S. S. Stevens promoted an interpretation of operationism that came to be conventionally accepted in psychology and that did just the reverse. Under the Boring-Stevens interpretation of operationism, researchers and theorists may deploy some public observation as a proxy or surrogate for an underlying causal mental phenomenon, all the while claiming they have met the requirements for good science because they have always confined themselves to publicly observable measures of the phenomenon. On this view, although mental concepts could not be *directly* admitted to psychological science, they could be *indirectly* admitted, provided that they were operationally rendered using publicly observable measures that achieved agreement (e.g., Moore, 2008, p. 42; 1999, p. 59). Worth noting here is that the conventional interpretation of operationism follows from referential, correspondence-based assumptions about verbal behavior (Skinner, 1945, p. 270).

Skinner (1945, 1974) noted two problems with methodological behaviorism. The first is that taken literally, the position means we cannot consider such cases as how individuals come to use subjective terms, as when they tact pains as sharp or dull. We end up excluding something legitimate from psychological science.

The second problem with methodological behaviorism follows from the attempt to overcome the first through the Boring-Stevens interpretation of operationism and its attendant appeal to publicly observable data to support explanatory concepts. The second problem is that researchers and theorists may uncritically operate with an underlying mentalistic if not dualistic orientation but assume that they are operating correctly. Thus, concepts of ancient and nonscientific origin can be retained and insulated, to the detriment of progress. Spurious independent variables can be cited, while actual ones are not identified. The result is that possibilities for prediction

and control are forsaken. Regrettably, Baum's treatment of private events as expressed in his review failed to even recognize, let alone address these problems. Skinner's did. On this basis I decided to include Skinner's treatment in my book. On this basis, Baum's treatment requires considerable clarification.

Thus, we have it that in one passage, Baum (2010) stated as follows:

A person in pain must do more than say "My tooth hurts." He must grimace, hold his face, be distracted, and ultimately be relieved by aspirin or the dentist. All of these public events, or at least some of them, must be present before we unhesitatingly agree that the person is in pain. A person who claims to be in pain but exhibits no other pain-behavior is, for all practical purposes, not in pain... (p. 123)

This passage, especially the last sentence, is as clear an illustration of methodological behaviorism as I can imagine. I regret I do not see the logical force of the three instances of "must" in the passage above, or the reason that speakers who claim to be in pain cannot be considered to actually be in pain if listeners do not observe them engaging in some prescribed form of nonverbal behavior. This stance equates verification by a listener with the states of affairs that actually cause the behavior of a speaker. I agree that listeners may not trust speakers who talk of being in pain if the speakers have little tendency to concomitantly engage in nonverbal public pain behavior. I agree that in conventional circumstances public pain behavior of speakers is a criterion for ascribing pain to the speakers. I agree that just saying "My tooth hurts" does not guarantee the presence of a toothache—the speaker could be faking, as Baum correctly noted. However, speakers can clearly be painfully stimulated without engaging in nonverbal behavior that listeners can see. Importantly, how is it that speakers come to describe their pains? The answer comes from a causal analysis of the verbal behavior of the speaker (Skinner, 1945, p. 294). Baum says nothing about such an analysis.

For example, I wonder how Baum would account for the personal experiences that would lead him, for example, to inform Rachlin that the sensation he is experiencing at some particular moment is in fact a sharp pain of a toothache rather than a dull pain. Knowing the kindly Professor Rachlin as I do, let me assume

he would on such an occasion hand Baum an aspirin, which I hope for Baum's sake would provide some negative reinforcement—I would not want Baum to be grumpy about one more thing. Would Baum say that Rachlin would not have handed him the aspirin until he had observed Baum's actions over some temporally extended period of time, after which Rachlin would have required Baum to put his hand to his jaw? Would Baum say Rachlin would have denied Baum's pain because Baum had only talked of it, and not engaged in any other form of publicly observable behavior like putting his hand to his jaw? Would Baum actually say he was immune to the pain of a toothache because Rachlin was not in the room to verify he was moaning and groaning, putting his hand to his jaw, looking for an aspirin, or seeking an appointment with a dentist? It seems to me that Baum would have to answer in the affirmative to these admittedly rhetorical matters.

In reply, I suppose Baum could cite temporally extended interactions with Rachlin since the 1960s that have established their mutual credibility. But where are Rachlin's temporally extended observations related to Baum's tact of the pain as sharp? If the meaningfulness of statements about private stimulation can only be derived from their molar relation to observable behavior, I am unclear how Baum's current verbal emission of sharp versus dull is related in any obvious way to the temporally extended observation of his behavior. Observable events were presumably related in the past, as often speakers learn to tact their pain as sharp when it is caused by a sharp object and dull when it is caused by a dull object. However, that is precisely Skinner's account of how the verbal community circumvents the problem of privacy to differentially reinforce responses to private stimuli, and ironically we have seen Baum wanted to distance himself from Skinner's supposed mistakes about private events, as well as mine.

As elsewhere, I submit that Baum's conceptions about the nature of private behavioral events are simply incoherent. It seems to me that in his version of psychological science, he must either (a) deny he has private events; or else (b) admit he has them but then rule them out of consideration, in favor of only publicly observable behavior because of concerns about scientific respectability; or else (c) treat them as ontologically nonbehavioral but nevertheless

causal when it comes to verbal behavior. None of these possibilities seems defensible.

Recapitulation

Baum's criticisms of my book's treatment of private behavioral events are derived from his own particular view of how they *should* be treated. Yes, my view differs from his, manifestly and deliberately so. Baum has failed to show why his own treatment of private events (and Rachlin's, for that matter) is not garden-variety methodological behaviorism circa the 1940s, and particularly why my book's treatment of private events is inadequate. Indeed, in response to a direct question from me, Rachlin (personal communication, April 7, 2000) once explicitly and unselfconsciously acknowledged that his position was nothing but methodological behaviorism. In Baum's revision, I recommend he reconsider the implications for the methodological behaviorism inherent in his position. For that matter, I recommend he invite Rachlin to look over his shoulder as he does so. To paraphrase Skinner (1945, p. 294), it is an amusing bit of irony that while Baum and Rachlin must confine themselves to an account of my external behavior, I am still reasonably interested in boring within their skins to elucidate any forms of discriminative private stimulation we might find there.

SUMMARY AND CONCLUSIONS

I agree with Baum (2010) that a central feature of my book is its focus on mentalism and verbal behavior. After all, each of us does have a "web of belief" or set of assumptions that guides our behavior (e.g., Quine & Ullian, 1978), and it appears that mine differs decidedly from Baum's. In light of this difference, it appears that Baum's (2010) fundamental concern is that my book is not the book he would have written, on the topics he would have chosen, with the individuals and sources he would have cited, with the explanatory concepts he would have deployed. Therefore, he argued that it is not the book I *should* have written, and is not now a book others *should* read, or at best it is a book others *should* read only advisedly, after paying close attention to his review. Of course, I may be understating his concern here, but given the relentlessly disparaging posture of his review, I think not. I wonder if readers want more from

him, as I suspect they would from me, than just a statement of personal prejudices.

My book outlined the conceptual foundations of a science of behavior. Its largely expository perspective seeks to prepare readers to evaluate both historical and contemporary issues for themselves. For Baum to criticize my book because it does not conform to the preconceptions of his web of belief strikes me as exceedingly curious in light of the intellectual ability he has exhibited elsewhere. Perhaps Baum would have preferred a book titled *A Survey of Selected Conceptual Issues in One Form of Behaviorism as Defined by the Work of Several Individuals Who Graduated From the Harvard Psychology Department in the 1960s With Ideas That Sought to Remedy What They Unilaterally Decided Were the Mistakes of B. F. Skinner*. I confess I am not immediately clear just how much of Skinner's writings these individuals actually understand, and how the scope of such a book would differ from Baum (2005), Rachlin (1994), or even Staddon (2001), notwithstanding Baum's (2004) aforementioned reservations about the way Staddon made his case—readers will recall Baum said he largely agreed with the essentials of Staddon's case against Skinner. In any event, these authors have all had their say. It is useful to consider a different point of view.

A reasonably important starting point for discussions about theoretical, philosophical, and conceptual foundations of a science of behavior is a thoroughgoing functional, behavioral orientation to verbal behavior. Perhaps this is why Skinner considered *Verbal Behavior* (Skinner, 1957) to be his most important work. In turn, a behavioral conception of verbal behavior leads to a behavioral conception of scientific verbal behavior. In turn, a behavioral conception of scientific verbal behavior leads to a behavioral conception of epistemology. The functional analysis of the behavior of participants in psychological research is surely important, but so also is a functional analysis of the knowledge claims scientists make about the behavior of their participants. The behavior of scientists is to be accommodated in the same terms as the behavior of participants. My book sought to follow this path, culminating with epistemology in the last chapter, however briefly.

I see no evidence in Baum's review of anything like the functional orientation to

verbal behavior that lies at the heart of radical behaviorism. Some, but not all, of the contemptuous negativity in Baum's review stems from the obvious differences between my functional orientation and his orthodox orientation that leads on to the methodological behaviorism and a mentalistic epistemology that radical behaviorists have long argued against. I confess I am curious why Baum embraced Stevens instead of Skinner and unhesitatingly took such a pronounced methodological behaviorist view. I expected better. Perhaps readers did as well. Verbal behavior arising from methodological behaviorism is of concern because it just does not occasion very effective action in the world at large. It owes more of its strength to social and cultural considerations than to materially beneficial consequences.

Baum is certainly entitled to clarify his position in a scholarly way. I invite him to do so, and not produce more of the same verbal behavior. For my part, I prefer to work with him to establish a common foundation for progress, as challenging as that may prove to be in light of the path he chose when he reviewed my book. The increased probability of effective action is what shifts paradigms. The probability increases when independent variables are identified in a causal analysis based on contingencies of reinforcement. Analysis of the verbal behavior of researchers and theorists plays an important part in the ultimate story.

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